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THE CHEMISTRY OF THE FUTURE

Ideal Chemistry. By Sir B. C. Brodie, Bart., D.C.L., F.R.S. A Reprint of a Lecture delivered before the Chemical Society on June 6, 1867. (London: Macmillan and Co., 1880.)

CHEMISTS who wish to study the "Calculus of Chemical Operations" will value this reprint of the lecture delivered by Sir Benjamin Brodie shortly after presenting his first memoir on the subject to the Royal Society, as it is in the main devoted to the description and explanation of the special symbols employed in the Calculus.

Even if this were the time and place, I should not venture to submit Sir Benjamin Brodie's views to that exhaustive analysis, which I believe has hitherto never been accorded to them, but which they must ere long receive at the hands of chemists. As yet only portions of the Calculus have been published, viz., Part I. "On the Construction of Chemical Symbols," and Part II. "On the Analysis of Chemical Events," although a valuable supplementary explanation of certain features was recently elicited by Naquet's criticisms. We have still to learn how the author proposed to treat of isomerism, by far the most intricate and difficult problem yet to be solved in chemistry, and let us hope that his departure from amongst us, which we now deeply lament, may not involve the suspension of judgment on this point he asked for but a short time ago being for ever.

I cannot refrain from devoting this notice to specially directing attention to what appears to me to be the topic of fundamental importance in the lecture, viz. the suggestion, made the author believes for the first time excepting in a few words at the conclusion of his first Memoir in the *Philosophical Transactions*, of the possible decomposition at the elevated temperature of the sun of certain chemical elements.

The fundamental hypothesis of the Calculus is to express the symbol of the unit of hydrogen by one letter, a. Hydrogen is to be regarded as constructed at once, by one operation. But while hydrogen is conceived of as the product of a single operation, the hypothesis indicates that oxygen, ξ^2, cannot be conceived of as made by less than two operations; while chlorine, $a\chi^2$, and nitrogen, $a\nu^2$, for example, are each to be conceived of as made by three operations, one operation in each case being that by which hydrogen is made. In short, the hypothesis involves the conclusion that there are several distinct classes—three at least—of "elements," of which hydrogen, oxygen and chlorine are the types, formed respectively by a single operation, by two similar operations, and by several operations not all similar. In other words, to quote the author, "we are led to a certain physical hypothesis as to the origin and causes of chemical phenomena." He then continues:—

"Now what I am going to suggest you must consider to be put before you with reservation, but we may conceive that in remote time, or in remote space, there did exist formerly, or do exist now, certain simpler forms of matter than we find on the surface of our globe—a, χ, ξ, ν, and so on—I say we may at least conceive of, or imagine, the existence in time and space of these simpler forms of being, of which we have some records remaining to us in such elements as hydrogen and mercury. We may consider that in remote ages the temperature of matter was much higher than it is now, and that these other things existed then in the state of perfect gases—separable existences—uncombined. This is the farthest barrier to which in the way of analysis theory can reach. Beyond all is conjecture. There may be something further, but if so, we have no suspicion of it from the facts of the science. We may then conceive that the temperature began to fall and these things to combine with one another and to enter into new forms of existence, appropriate to the circumstances in which they were placed. We may suppose that at this time water ($a\xi$), hydrochloric acid ($a\chi$), and many other bodies began to exist. We may further consider that as the temperature went on falling, certain forms of matter became more permanent and more stable, to the exclusion of other forms. We have evidence on the surface of our globe of the permanence of certain forms of matter to the exclusion of others. We may conceive of this process of the lowering of temperature going on so that these substances, $a\chi^2$ and $a\nu^2$, when once formed, could never be decomposed—in fact, that the resolution of these bodies into their component elements could never occur again. You would then have something of our present system of things."

We have here a most distinct prior statement by Sir Benjamin Brodie of views almost identical with those which have been so persistently urged for several years past by Mr. Lockyer, whose arguments, however, have hitherto met with but little sympathy from chemists, mainly perhaps on account of the unwonted character of the evidence. In his paper read before the Royal Society in December 1878, Mr. Lockyer adduced two lines of evidence in support of his hypothesis of elemental dissociation at high temperatures: The existence of lines common to several spectra—so-called basic lines—and the progressive alteration in the character of the spectra of the stars with temperature. Neither of these lines of argument has, I believe, yet been impugned, and the criticisms launched against the hypothesis have been on side issues of no real importance to the main question under discussion. More recently additional evidence in the same direction has been obtained by the comparison of the observations of Tacchini and others on solar storms. It appears that whereas at certain times lines which are admittedly all iron lines are visible, at other times certain of these lines are wanting from the spectrum, new lines appearing in their place: fluctuations of this kind taking place at frequent intervals, but evidently in accordance with some well-defined law. Facts such as these may after all meet with some other interpretation than that furnished by the "dissociation" hypothesis, although at present this affords by far the simplest explanation of them. A communication of Mr. Lockyer's, read at the last meeting but one of the Royal Society, however, adduces evidence which if confirmed must, it would seem, be regarded as final. It is well known that the velocity of uprush or downrush of vapours at the sun may be determined by observations of the amount of displacement from their normal position of the lines in the spectrum of the vapours, and obviously if all the lines in a given spectrum—that of iron, for instance—are lines due to one substance, it must be a matter of indifference by which of the lines the velocity is measured. Whereas, on the other hand, if this be not the case, and the simpler substances into which the body

is split up be of different degrees of volatility—of different molecular weight—we may expect that measurements of the displacement of different lines will not all furnish the same results. Mr. Lockyer states that in an observation of a sun-spot on August 31 of this year, when the iron line at λ 5207·6 was doubly contorted, indicating an ascending and descending velocity of about fifteen miles a second, the two adjacent iron lines at λ 5203·7 and 5201·6, visible in the same field of view, were perfectly steady. Observations of this kind are necessarily very difficult, and the communication is made with all reserve; but it is to be hoped that observers elsewhere will co-operate in at once putting this observation to the test.

It is difficult to exaggerate the importance of the question to the chemistry of the future, for should it once be proved that the dissociation of the so-called elements is taking place in the sun and still hotter stars, it will be within the power of the physicist with the aid of the telespectroscope to build up a theory of elemental evolution not inferior in interest to the doctrine of organic evolution. For my part, I have no fear of the result, for apart from Sir Benjamin Brodie's hypothesis and apart from spectroscopic evidence, I believe that in the relations of the "elements" to each other when arranged more or less in accordance with the now well-known periodic law of Mendeljeff we have distinct proof of progressive development, but of this I hope to say more on another occasion.

Sir B. Brodie points out in his lecture that if the symbol a'^2 were assigned to hydrogen, instead of the symbol a, a different symbolic system analogous in its form to the system in vogue amongst chemists would result In the second part of the Calculus he has fully explained his reasons for adopting the hypothesis a, notwithstanding that it leads to conclusions so entirely different from those ordinarily accepted, the chief reason being that this hypothesis satisfies the so-called law of even numbers—the law that the sum of all the units of affinity in a compound is an even number. The recent remarkable discovery—probably one of the most important theoretically ever made by chemists—of the behaviour of the halogens at high temperatures would appear to furnish an opportunity of experimentally ascertaining whether Sir B. Brodie's hypothesis a is admissible, for this hypothesis would not admit of a simple resolution of the diatomic molecules of chlorine, bromine and iodine into monatomic molecules which has been regarded as the more probable explanation of the results obtained by Victor Meyer and by Meier and Crafts. Two well-established exceptions to the law of even numbers exist, nitric oxide, NO, and nitric peroxide, NO_2, but as is well known, Sir B. Brodie has suggested that in these we may not be dealing with homogeneous gases, but that each is constituted of two gases which, taken together, are made up of oxygen and nitrogen, but which separately are not so made up: hypothesis a would lead to similar conclusions regarding the constitution of chlorine, bromine and iodine at high temperatures.

At present all that is *established*, however, regarding the halogens is that iodine begins to undergo dissociation at a temperature between 600° and 700°, and that its vapour gradually diminishes in density until at a white heat it attains not far short of half the "normal" value. Whatever the nature of the dissociation products, the occurrence of dissociation must be regarded as *placed beyond doubt*, for Victor Meyer's results have been in the main confirmed not only by Meier and Crafts, but also by Deville and Troost, who had previously obtained normal results. Bromine does not undergo dissociation so readily as iodine, the ratio of the observed to the theoretical "normal" density being, according to Meier and Crafts, ·8 for bromine when it is ·66 for iodine. In a recent communication, Victor Meyer has stated that the results of his earlier experiments with chlorine would appear to have been vitiated by some as yet undiscovered source of error; this gas probably is not dissociated except at extremely high temperatures, and it is doubtful whether there is any difference in behaviour between free and nascent chlorine.

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HANDBOOK OF BOTANY

Handbuch der allgemeinen Botanik. Von Prof. Dr. N. J. C. Müller. Zweiter Theil. Allgemeine Morphologie und Entwickelungslehre der Gewächse. Pp. 482, Figs. 277. (Heidelberg, 1880: Carl Winter's Universitatsbuchhandlung.)

THIS is the second instalment of a work by a single author which is to treat of all the different departments of botanical science. The first volume, which is devoted to the consideration of the Physiology and General Anatomy of Plants, was reviewed in NATURE, vol. xxi. p. 589. It is impossible to pass a more favourable verdict upon this volume than upon its predecessor. It is characterised by the same failing, namely, a want of clearness and definiteness in the statement of important facts and fundamental principles. The first section of the book is devoted to a discussion of the theory of descent, the origin of species, and the occurrence of varieties and monstrosities, with the object, presumably, of making the reader acquainted with some, at least, of the influences which determine the forms of living organisms. The account of the morphology of plants begins at p. 38, and after ten pages of general considerations the subject is actually grappled with. Prof. Müller commences with the Thallophytes, though he does not call them so, for his first section on them is headed "Der Algenstamm." It is not easy to understand what he means by the suffix "stamm"; does he mean to describe the thallus of the Alga as being a "stem," or does he use the word in the sense of "tribe"? Whichever be the true interpretation, it still remains unexplained why this word should appear as the heading of a section which treats not only of Algæ, but of Fungi as well. The prospectus of the work sets forth that the Classification of Plants is to form the subject of a subsequent volume, and there is therefore some hope that Prof. Müller will there give a classification of the Algæ which is more in accordance with facts and with reason than the one which he now follows. It is impossible to imagine on what grounds the Palmelleæ, the Protococcæ, and the Volvocineæ should be united together to form the Order Palmellaceæ, and yet this is done on p. 51 of this work, although the author is evidently aware of the fact that in Volvox reproduction is effected by means of sexually produced oospores, as his account of that plant, a singularly inaccurate one be it said, on p. 62 testifies.